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The Role of Constraints in Expert Memory

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Abstract

A great deal of research has been devoted to developing process models of expert memory. However, Vicente and Wang (1998) have proposed that process theories have difficulty in explaining expert recall in domains in which memory recall is a contrived task, and that a product theory, the Constraint Attunement Hypothesis (CAH), has received a significant amount of empirical support. We compared one process theory (the template theory; TT, Gobet & Simon, 1996a) with the CAH in chess. Chess players ($N = 36$) differing widely in skill levels were required to recall briefly-presented chess positions which were randomized in various ways. Consistent with TT, but inconsistent with the CAH, there was a significant skill effect in a condition in which both the location and distribution of the pieces were randomized. These and other results suggest that process models such as TT can provide a viable account of expert memory in chess.

Keywords: Chunking, Computational modelling, Constraint, Environment, Expertise, Memory

The Role of Constraints in Expert Memory

Ever since Piaget (1954), Brunswik (1956), and Simon (1969), the environment has played an important role in psychological theories. However, while all psychologists agree that cognitive systems adapt to the structure of their environment, there are disagreements about the consequence of this on the optimal way to study cognition. On the one hand, it has been proposed that the best approach is to develop process theories, preferably in the form of computer programs, and compare their predictions with human data (e.g., Anderson & Bower, 1973; Newell, 1990; Newell & Simon, 1972). On the other hand, it has been argued that a careful study of the properties of the environment should come first, before any attempt to theorize about cognitive processes (Anderson, 1990; Brunswik, 1956; Gibson, 1966; Vicente & Wang, 1998). Once these properties are fully understood, developing process theories becomes an easier, more constrained task.

Recently, this debate has focussed on the “expertise effect” in memory recall. Experts are vastly superior to non-experts in memorizing briefly-presented material taken from their domain of expertise. This effect, first reported about fifty years ago in chess (de Groot, 1946/1978), has been observed in many additional studies, in domains including games, programming and medical expertise (see Ericsson, 1996; Vicente & Wang, 1998, for recent reviews). A number of process theories have been developed purporting to explain the cognitive mechanisms underlying this effect, including the chunking theory (Simon & Chase, 1973), the skilled memory theory (Chase & Ericsson, 1982; Ericsson & Staszewski, 1989), the long-term working-memory theory (Ericsson & Kintsch, 1995; Ericsson, Patel & Kintsch, 2000), and the EPAM-IV and template theories (Gobet & Simon, 1996a, 1998, 2000; Richman, Staszewski & Simon, 1995; Simon & Gobet, 2000). In contrast, Vicente and Wang (1998) proposed an ecological theory of expert memory – the constraint attunement hypothesis (CAH) – that focuses more on understanding the goal-relevant constraints in a

domain. The CAH is also a “product theory” of expert memory in that the functional relation between input and output is described, but - like all product theories - it is silent about specific psychological mechanisms.

In their review, Vicente and Wang (1998) made a distinction between domains in which memorizing stimuli is an intrinsic task (a task that is a definitive feature of that domain of expertise), and domains in which memorizing stimuli is a contrived task (a task that is not part of that domain of expertise). They argued that the process theories they discussed “cannot provide an adequate theoretical explanation for expertise effects in memory recall for the considerable number of domains in which memory recall is a contrived task” (pp. 34-35). In contrast, they noted (p. 46) that “all but one of the results reviewed in this section were consistent with the predictions generated by the constraint attunement hypothesis, thereby providing a significant amount of empirical support for the theory” and “[many of the results that were reviewed] still could not be accounted for, a posteriori, by other competing theories.” Vicente and Wang’s analysis is further discussed by Ericsson, Patel and Kintsch (2000), Simon and Gobet (2000), and Vicente (2000).

Here, we addressed this challenge in part by making a detailed comparison between one process theory (the template theory; TT) and the CAH in one domain, chess. (See Simon & Gobet, 2000, for a comparison across a range of domains.) We chose TT because it is a well articulated process theory of expertise and because it is discussed in detail by Vicente and Wang. We focused on chess because Vicente and Wang’s (1998) review contained six chess experiments (out of ten), and because – as will be seen later - they explicitly proposed an experiment where the predictions of the CAH and TT differ. Thus, a major goal of the paper was to compare the predictions of TT and the CAH under experimental conditions where they make different predictions.

We start by describing the CAH and TT. We then argue that the theories can be disentangled by examining recall for different types of randomized chess positions. Predictions of TT are derived from a simulation study. Predictions of the CAH are derived from Vicente and Wang (1998). Finally, we report a human study which tested these predictions.

The Constraint Attunement Hypothesis

The constraint attunement hypothesis (CAH) is derived from Gibson's (1969) specificity theory and Rasmussen's (1985) notion of abstraction hierarchy. It represents an outgrowth of ecological theories of cognition, which stress the importance of the environment over internal processes. Vicente and Wang (p. 36) stated the CAH as follows:

There can be expertise effects [in memory recall] when there are goal-relevant constraints (i.e., relationships pertinent to the domain) that experts can exploit to structure the stimuli. The more constraint available, the greater the expertise advantage can be. Fully random stimuli have no constraints, so no expertise advantage would be expected. To realize these potential advantages, experts must be attuned (i.e., they must attend) to the goal-relevant constraints in question. If they do not pick up on this information, then no expertise advantage is expected.

To define the goal-relevant constraints, Vicente and Wang propose that, for each particular domain, an abstraction hierarchy should be constructed (p. 36). In the abstraction hierarchy, there are constraints on relationships both within, and between, levels of the hierarchy. For example, one important feature of an abstraction hierarchy is that levels of the hierarchy are connected by means-end relationships (p. 37). Vicente and Wang claim that these types of constraint are the goal-relevant constraints that experts can exploit when recalling material within their domain.

Vicente and Wang (pp. 55-57) sketch an abstraction hierarchy for chess. This hierarchy has five levels: board, paths, tactics, strategies, and purpose. The lowest level, board, consists of the number and type of pieces as well as the physical properties of the board. For example, the number of pieces impose constraints on the possible physical configurations (e.g., not more than 16 pieces of the same color). The second level, paths, consists of the constraints resulting from the rules governing the movement of pieces. The next level, tactics, “is constrained by the meaningful and effective ways in which moves can implement goal-relevant strategies” (p. 56). About the next level, strategies, Vicente and Wang note that “masters do not constrain their moves by the rules of the game or tactics alone. Rather, their actions are also highly constrained by the higher order strategic plans that they have adopted to achieve a win (cf. Holding, 1985)” (p. 56). Finally, the highest level, purpose, will constrain the strategies that can be meaningfully chosen.

The CAH explains expertise effects for standard chess positions in the following manner. When an expert looks at a position s/he may attune to a strategic configuration (e.g., white attacking black with a wing attack), thereby ruling out other strategic factors. The strategic configuration constrains tactical features of the position (e.g., white may have stockpiled his or her pieces on a given file), which in turn will constrain the path level (certain moves will be expected). The expert, but not the novice, has the capacity to attune to these multiple constraints, allowing better recall. Vicente and Wang (1998, p. 57) put it as follows: “The links between levels define what is goal relevant for that domain, thereby allowing people who are aware of these relations to reduce the number of meaningful alternatives that need to be considered in reconstructive recall.”

The Chunking and Template Theories

Simon and Chase’s (1973) chunking theory (CT), which is closely related to the EPAM theory of memory and perception (Feigenbaum & Simon, 1984; Richman & Simon,

1989; Richman et al., 1995), was the first theory to precisely specify the cognitive mechanisms involved in chess memory. CT proposes that at the core of expertise lies the ability to rapidly recognize important features in the problem at hand, such as the location of groups of pieces on the board. These features are stored internally as chunks - symbols in long-term memory (LTM), having an arbitrary number of subparts and properties - that can be used as knowledge units. Chunks act as access points to semantic LTM and as the conditions of productions, whose actions may be carried out internally or externally. In the case of chess expertise, perceptual mechanisms in CT allow recognition of patterns of pieces on the board. These patterns suggest moves, which are used to update the internal representation of the board in the mind's eye. The mind's eye acts as a relational system which stores perceptual structures. These structures, which can originate both from external inputs and from memory stores, can be manipulated by visuo-spatial mental operations. Finally recognition mechanisms apply both when perceiving the external position and when examining the positions that are generated in the mind's eye during search. A subset of CT was implemented as a computer program by Simon and Gilmarin (1973).

Two weaknesses were later uncovered with CT: (a) in an interference paradigm, the theory underestimates speed of encoding into LTM (Charness, 1976); and (b) the theory does not relate mechanisms at the chunk level with the higher-level representations that players use (Cooke, Atlas, Lane & Berger, 1993; Holding, 1985). The template theory (TT) (Gobet & Simon, 1996a; 1998; 2000) was developed to eliminate these two weaknesses, while keeping the strength of the original chunking theory. TT is able to account both for perceptual phenomena, such as players' eye movements during short presentations of unknown positions, and memory phenomena, such as immediate recall of positions. The theory is implemented as a computer program (CHREST- Chunk Hierarchy and REtrieval STructures) that offers detailed simulations of the empirical data (de Groot & Gobet, 1996; Gobet, 1993,

1998; Gobet & Simon, 1996c; 2000). Finally, an extension of the program (Gobet & Jansen, 1994) plays (weak) chess by pattern recognition - recognized chunks elicit potential moves or sequences of moves.

Like CT, TT proposes that chunks are accessed by traversing a discrimination net. A discrimination net consists of a set of nodes (chunks) connected by links, which together form a treelike structure. The nodes have tests, which can be applied to check features of the external stimuli. The outcome of each test determines which link will be taken below a node. This net is grown by two learning mechanisms, familiarization and discrimination, which together produce a self-organizing, dynamical system. When a new object is presented to the model, it is sorted through the discrimination net, starting from the root node, until no further test applies. When a node is reached at the end of this process, the object is compared with the image of the node, which is the internal representation of the object. If the image underrepresents the object, new features are added to the image (familiarization). If the information in the image and the object differ on some feature or some sub-element, a new link and a new node are created below the current node (discrimination).

Chunks are linked to other information stored in LTM, such as moves, plans, and tactical motives. (In its current implementation, CHREST acquires only information about patterns of pieces and sequences of moves.) Moreover, chunks that are often used in a player's practice evolve into more complex data structures (templates), which have slots allowing variables to be instantiated rapidly. In particular, information about piece location, piece type, or chunks can be (recursively) encoded into template slots. Slots are created at chunks where there is substantial variation in squares, pieces, or groups of pieces in the test links below. In addition to slots, templates contain a core, basically similar to the information stored in chunks. Altogether, templates typically store about ten pieces, but the number of pieces can be much larger, more than 20 in some cases.

Templates may be linked to other templates, allowing search at a higher level than moves (i.e., planning). TT also proposes that the information stored in the mind's eye decays rapidly, and that it needs to be updated either by inputs from the external world or by inputs from memory structures. Finally, TT, like CT, proposes that search is carried out by recursively applying pattern recognition processes to the internal representation (Gobet, 1997, 1998).

Memory for Random Chess Positions

Given that the amount of constraint in the chess stimulus is central to the CAH, one way to examine the CAH is to manipulate this variable and to observe the effects on recall. The amount of constraint can be manipulated by various randomization procedures. Indeed, historically, random material has been one of the most powerful tools for studying experts' memory in contrived tasks (see Gobet, 1998). Importantly, Gobet and Simon (1996b, 1996c) have shown that chess experts keep some superiority with random positions, even if the skill advantage is much less than with game positions. As simulated by CHREST (Gobet & Simon, 1996c, 2000), this result fits the predictions of the chunking and the template theories. Both these theories assume that experts acquire a large number of perceptual chunks and that they are therefore more likely than non-experts to pick up patterns that occur by chance in random positions.

At first sight, it might be thought that the presence of expertise effects in recall of random positions should disentangle the CAH and TT. If a position is random, there are presumably no goal-relevant constraints that can be exploited by experts, and therefore the CAH should not predict an expertise effect. However, Vicente and Wang (1998) note that published studies have employed a randomization process where the location of the pieces on the board is randomized, but not their distribution. In most experiments, the random positions were generated by randomizing the pieces of positions from master-level games

after about 20 moves. Their insight is that these master-level games may have statistical properties at the board level that will be inherited by the random positions. For example, suppose that a master-level position after 20 moves has an 80% chance of containing one white queen and one black queen. The experts - who know the board level constraints of master-level games - may be able to exploit this constraint to aid recall. In contrast, the weaker players cannot exploit these constraints. Therefore, both the CAH and TT predict a small skill effect on classic random positions and cannot be distinguished with this material.

However, Vicente and Wang (1998) identified a type of random position where the CAH makes a different prediction to TT (p. 45):

“....our observation leads to a new experiment that contrasts our theory with that of Gobet and Simon (1994, 1996b). The two theories make different predictions for a truly random chess position, in which both the position and the selection of the pieces are randomly determined (as far as we know, this procedure has not been adopted before). Gobet and Simon (1996b) predicted a small but significant expertise effect for completely random positions, whereas our theory would predict no expertise effect.”

We tested their prediction in this study. Figure 1 shows the algorithm we used to generate truly random positions, and Figure 2 shows an example of a position (called 3/3 truly random) generated by this algorithm. As can be seen, this position violates the most basic board level constraint of chess - that a position has one white king and one black king. In addition, the position contains other unusual features such as three black knights (two or less would be expected), and three white bishops (two or less would be expected). Thus, unlike the classic random stimuli, these positions do not inherit the statistical properties of master-level positions. There are no constraints that can be exploited by the masters but not by the weaker players. Thus, the CAH predicts no skill effect in recall of these positions,

while TT predicts a small skill effect (we shall later quantify this effect), since experts are still more likely to recognize more chunks in these positions than non-experts.

Overview of Experiments

We first address the effect of true randomization on recall with a computer simulation. This allows us to formalize TT's predictions about the relationship between skill and recall on truly random positions. Additionally, we included positions where only one-third or two-thirds of the pieces are truly randomized, which allows more fine-grained testing of TT's quantitative predictions (as well as the CAH's ordinal predictions). We selected three levels of true randomization (i.e. one-third, two-thirds, fully randomized) as we wished to examine the relationship between degree of randomization and template access by TT's large nets (and by the strong human players), while at the same time being careful not to overburden the human participants with too many experimental conditions (and therefore trials). This was particularly relevant as we also included game and random positions that are typically used in expert memory studies in chess. In the human experiment that follows, chess players have to recall the same types of positions as were presented in the simulations. To anticipate the conclusion of this paper, we will show that, in the case of memory for chess positions, both TT and the CAH make a number of correct predictions, but in the truly random condition proposed by Vicente and Wang, TT's prediction – but not the CAH's - is supported.

Computer Simulation Study

As noted above, Vicente and Wang (1998, p. 45) proposed an experiment to discriminate the CAH and TT. The idea was to use “truly” random positions where not only the location of the pieces is randomized, but also the distribution of pieces. The CAH predicts no skill difference for these positions, while TT predicts a small skill difference. As indicated above, we extended their proposed experiment by using positions where 1/3 of the pieces are truly randomized, and positions where 2/3 of the pieces are truly randomized. These

additional position types are useful for generating further predictions from TT and the CAH. As noted above, we also included game and random positions that are typically used in chess memory studies.

Method

Materials

Five types of position were used. They will be referred to as game, random, one-third truly random (1/3), two-third truly random (2/3), and truly random (3/3) positions. Five hundred stimuli were selected by randomly sampling (without replacement) from a database of 3,100 positions taken from master-level games after about 20 moves. In the game condition, the stimuli were kept unchanged. The four types of random positions were generated using the algorithms described in Figure 1, and examples of all position types are shown in Figure 2.

Procedure

As the template theory is implemented as a computer program (CHREST), it is simple to run it on the material proposed above and to obtain quantitative predictions. We used exactly the same version of CHREST as used by Gobet and Simon (2000) - no parameter was altered to fit the data. We created 16 nets, having from 100 to 300,000 nodes, by letting the program scan a database of about 50,000 positions taken from master-level games (the positions were middle game positions, taken from games played in the last fifty years). During the learning phase, the program moved its simulated eye around the board, and attempted to learn the patterns within its visual field (± 2 squares from the fixation point) using the mechanisms of familiarization and discrimination. Template slots were created when two conditions were satisfied: (a) the number of nodes below a given node that share identical information (either a square, a type of piece, or a chunk) is greater than 3; and (b) the node to which a slot could be added contains at least 5 elements. Slots could encode only

information referring to their type: that is, either to kind of piece, location, or chunks having a common sub-chunk (see Chapter 8 of de Groot & Gobet, 1996, and Gobet & Simon, 2000, for details on the model).

During the testing phase, each of the 16 nets was used to simulate the recall of 500 positions of each type. Each position was presented for a simulated time of 5 s. During the presentation of the position, the model moved its simulated eyes around the board, and attempted to recognize chunks (or templates). In case of successful recognition, a pointer to the chunk was placed in visual short-term memory (STM), the capacity of which was set to 3. During the 5 s of presentation, the model could add information to LTM in two ways: by familiarization (augmenting the internal representation of a pattern), which took 2 s, and by filling in information into a template slot, which took 250 ms. The third learning mechanism, discrimination, could not be used by the model, because its execution takes longer than the 5 s presentation time. All time parameters were the same as in the simulations reported by Gobet and Simon (2000). Note that TT's mechanisms are the same in all position types (from game to truly random). Thus, any difference in recall performance reflects the probability that external patterns will elicit chunks or templates in LTM.

Data Reduction and Analysis

We selected four of the sixteen nets (with 1,010, 3,008, 15,003, and 300,009 chunks) which most closely matched the mean recall of four groups of human subjects on game positions (to be reported in detail later¹). To facilitate comparisons with the human data, the 300,009 chunk model was considered to have a rating of 235.6 in "human" units (the mean skill rating of our top group of humans). That is, we treated the 300,009 chunk model as a computer participant with a rating of 235.6. The 15,003 model was considered to have a

rating of 201.2 in human units (the mean skill rating of our second group of humans), the 3,008 model a rating of 150.9, and the 1,010 model a rating of 112.3.

We derived estimates of the skill slopes in each position type in the following manner. We used linear regression to predict recall performance (the dependent variable) from the ratings (the independent variable). We did this for each position type (i.e, game, 1/3, 2/3, random, 3/3) separately. We used the unstandardized parameter estimates from these models as an index of the skill slope in each position type, and used the P value of the parameter estimate to determine whether the slope was significantly different from zero. To determine whether the skill slope on one position type was steeper than that on other position types, we used regression analysis (proc glm in SAS) to test for skill by position type interactions, where position type was entered as a repeated measures variable. To compare the skill slope on the 1/3 positions with the mean slopes on the other random positions, we used the helmert comparison option in SAS. All statistical analysis was carried out using SAS (SAS/STAT Software, 1997). Given the limited sample size, 1-tailed tests were used.

Results

Figure 3 (top) shows the recall performance of the four selected nets on the different position types. In general, recall improves as a function of the number of chunks (because more and bigger chunks can be accessed with larger nets), and decreases as a function of the amount of randomization (because, as randomization increases, the likelihood of matching a chunk decreases). The estimates of the slopes in the different position types were: game = 0.331 (SE = 0.032), 1/3 = 0.216 (SE = 0.058), 2/3 = 0.077 (SE = 0.021), random = 0.071 (SE = 0.016), and 3/3 = 0.061 (SE = 0.011); the skill slopes for all position types were significantly different from zero (all P's < .05). For the recall of truly random positions, the parameter estimate (0.061) indicates that an increase in skill level of 100 human units (about

the difference between an average club player and a grandmaster) yields an increased recall performance of 6.1%, or roughly 1.5 pieces.

Regression analysis indicated that the skill slopes on the four randomized position types (1/3, 2/3, random, 3/3) were significantly different from each other [Skill by Position Type interaction: $F(3, 6) = 11.1$, $P = .004$] (data from the game position types were not included in this analysis). Figure 3 indicates that the skill slope for the 1/3 positions is steeper than for the other randomized positions; statistical analysis confirmed that the skill slope on the 1/3 position types was greater than the mean slopes on the other three random position types [$F(1,2) = 11.6$, $P = .04$]. The slopes on the random and 3/3 position types did not differ significantly from each other [$F(1,3) = 1.36$, $P = .18$]; the same was true for the 2/3-3/3 comparison [$F(1,3) = 2.01$, $P = .15$].

Discussion

The purpose of the simulation study was to formalize TT's predictions. As expected, TT predicts a skill effect in all position types, including the truly random positions. TT also predicts that skill effects become smaller with randomization. This Skill by Position Type interaction is mainly explained by the fact that (a) large nets make it possible for CHREST to access more and larger chunks, and thus to obtain better recall performance, and (b) chunks get harder to find as positions get more random. The abrupt change in slope with game and 1/3 position is explained by the fact that CHREST can find templates and use their slots to fill in either the piece location or its type rapidly in game positions, and to some extent, in 1/3 positions, but not in the other types of positions. With smaller nets, CHREST can rarely find templates, even in game positions, and has to rely mainly on chunks.

There are only small differences between the skill effects on the 2/3, random, and 3/3 position types, and they do not reach statistical significance. Thus, taken at face value, TT predicts no difference in skill slopes on these three position types. However, we note that

these analyses had limited power to detect significant position type by skill interactions. In any case, the difference in slopes between the 1/3 and 3/3 conditions should be greater than that between the 2/3 and 3/3 conditions, and the random and 3/3 conditions.

Human Study

The main purpose of the study was to compare TT's and the CAH's predictions with human data. As indicated above, TT predicts a skill effect on all position types, including the truly random positions. In contrast, the CAH does not predict a skill effect on truly random positions. It does predict skill effects on the other position types.

On the randomized positions, TT predicts that skill effects become smaller in the following manner: 1/3 (predicted slope = 0.212) > 2/3 (predicted slope = 0.077) > random (predicted slope = 0.071) > 3/3 (predicted slope = 0.061), and that the slope on the 1/3 positions is greater than the mean slope on other randomized position types. The CAH does not give quantitative estimates of the skill effects but predicts that the ordering of the skill slopes should be as follows: Game > Random > 3/3, and Game > 1/3 > 2/3 > 3/3. As indicated earlier, the CAH predicts that skill effects should be greater in random positions than in the truly random positions, because the random positions inherit constraints that benefit the masters more than the other players. The CAH does not make any straightforward predictions as to whether the skill slopes for the random positions should be smaller or greater than those in the 2/3 (and 1/3) position types.

In sum, the main goal was to examine whether skill effects were present in truly random positions, as predicted by TT but not the CAH. We also evaluated the other predictions of TT and the CAH about the size and ordering of skill effects in different position types.

Method

Participants

Thirty-six participants completed the study. All participants were active chess players who had BCF (British Chess Federation) ratings in both the August 1997 and August 1998 ratings list. Nineteen participants also had international ratings in the FIDE July 1998 ratings list². Table 1 shows the characteristics of the sample by skill level. We split the participants into four groups. The top group (grandmasters, $N = 7$) were players with ratings of above BCF 225. The second group (masters/experts, $N = 12$) had BCF ratings between 175 and 224. This group contained three international masters, one FIDE master, one female grandmaster, one female international master, and six experts. The third group (Class A and B players, $N = 10$) consisted of players with ratings between 125 and 174; these players can be thought of as moderate to strong club players. The final group (Class C and D players, $N = 7$) contained players with ratings less than BCF 125; these players are considered weak club players. The grandmasters were paid £40 for participating, the second group were paid £20, and all other players were paid £10. Since previous studies (e.g., Gobet & Simon, 2000) have shown that masters are not very keen to recall random positions, a monetary reward was used to try to keep players' motivation level high; for each skill level, the prize for the best player was the equivalent to the participation fee of this skill level. The name of the winners was kept confidential. The experiment lasted on average around one and a half hours.

Materials

Chess stimuli. The same five types of position were used as were used in the simulation study (see Figure 2). Twenty-five positions (with an average of 25 pieces) were taken from master games after about 20 moves, and were randomly assigned to one of the five types of positions for each player. There were thus five positions in each condition.

Presentation software and hardware. Chess stimuli were presented on a portable Apple MacIntosh computer using specialized software for presenting chess stimuli and recording responses (see Gobet & Simon, 1998, for a detailed description of the software used). Participants were required to use a mouse to select pieces, move pieces onto squares, and delete pieces. To go on to the next trial, the subject pressed an OK button on the top left corner of the computer screen.

Chess questionnaire. Participants completed a questionnaire assessing basic information on chess play in the past year, such as the number of tournament games played, the time since the last game played, and the average amount of time per week each player spent studying chess. As some players report they find it much easier to look at board positions (i.e., a position with pieces on a chess board) than chess diagrams (i.e., the representations used in this study), the questionnaire also contained the following question: “How easy do you find it to read chess positions presented in two dimensions (on a page or computer screen) compared with the three dimensional board positions?”

Computer questionnaire. Participants filled out a brief questionnaire assessing computer use in which they were asked to rate their ability at using a computer mouse on a seven-point scale from 1 (not skilled at all) to 7 (very skilled).

Motivation questions. Participants were asked to rate their motivation to perform the task, their interest in the task, and their irritation with the task on seven point Likert-type scales at the end of the game and random positions.

Visual memory test. All participants completed a test of visual memory (the Shape Memory Test (MV-1) of the ETS Kit of Factor-Referenced Cognitive Tests; Educational Testing Service, 1976). Ekstrom, French and Harman (1979, p. 68) reported that the cognitive tests within the ETS Kit exhibited good to excellent internal reliability, generally in excess of 0.70 (when computed by calculating the split-half correlation and adjusting using the

Spearman-Brown correction). Factor analyses indicated that the Shape Memory test loaded strongly onto a Visual Memory factor (Ekstrom et al., pp. 70 - 71).

Procedure

Participants first completed the chess and computer questionnaires, followed by the visual memory test³. Participants then completed a brief measure of competence at using the computer: the starting position was shown for 5 s on the computer screen, and they were subsequently required to reconstruct it as quickly and as accurately as possible on an empty chess board. This was followed by the recall task. On each trial, a position was presented for 5 s. The screen then was blank for 2 s, and then an empty chess board appeared. The participants were instructed to try to recall the positions as completely and as accurately as possible. They had unlimited time to make their response. The twenty random positions (4 position types x 5 stimuli) were first presented in a different random order for each subject. After participants completed the motivation questions, the five game positions were presented, also in a different random order for each subject, and participants completed the motivation questions once again. Participants had two practice trials on random positions, and one practice trial on game positions⁴.

Data Reduction and Analysis

We used participants' ratings in the BCF August 1998 rating list as an index of chess skill, as this rating reflects performance in games most proximal to the completion of the experiment. We used Pearson's r to test the relationship between skill and recall on the different conditions. To obtain estimates of the skill slopes, we performed regressions in which recall performance (the dependent variable) was predicted from skill, age and visual memory entered together (the independent variables). Age was entered into the models because previous research has shown age to be an important variable in chess memory (Charness, 1981a, b), and because age could plausibly be related to recall on this task (Schulz

& Salthouse, 1999), even under the relatively restricted range of the study sample (age range 18 – 47). In addition, controlling for age is particularly relevant in this study as age and chess skill were correlated in our sample ($r = 0.38$, $P = .02$), with the better players being older. Thus, age effects could potentially reduce the size of the association between skill and recall performance; by including age in the regression models we were able to obtain estimates of skill slopes when statistically controlling for any effects of age. Visual memory performance was included in the regression models because general visual memory ability could plausibly be associated with both chess skill (e.g., Frydman & Lynn, 1992) and recall performance on this task (Waters, Gobet, & Leyden, 2002), and so it is important to demonstrate that any relationship between chess skill and memory recall is independent of general visual memory ability. Indeed, controlling for general visual memory ability is particularly important in this study since TT predicts skill effects (of different magnitude) in all position types, thereby leaving no position type in the study to serve as a control for general visual-spatial memory ability. Separate regressions were carried out for each position type. The unstandardized parameter estimate of the skill variable (BCF rating) provided our estimate of the skill slope, and the P value of this statistic determined whether it was significantly different from zero. To determine whether the skill slope on one position type was steeper than that on another position type, we tested skill by position type interactions, where position type was entered as a repeated measures variable with two levels. To compare the skill slope on the 1/3 positions with the mean on the other random positions, we used the helmert comparison option in SAS. Since the model makes directional and unambiguous predictions about recall performance on all conditions, we used 1-tailed tests for all analyses of skill effects on recall; 2-tailed tests were used for effects of age and visual memory on recall, and for all other analyses.

Results

We first examined the data on game and random positions; the recall percentages were 83.7 ($\underline{SD} = 14.4$), 61.3 ($\underline{SD} = 19.4$), 55.1 ($\underline{SD} = 17.2$), and 40.1 ($\underline{SD} = 10.1$), on the game positions and 18.3 ($\underline{SD} = 6.95$), 17.0 ($\underline{SD} = 6.25$), 14.1 ($\underline{SD} = 4.71$), and 12.3 ($\underline{SD} = 5.55$), on the random positions, for the grandmasters, masters/experts, Class A/B players, and Class C/D players, respectively. These values are typical of those that have been observed in the literature (e.g., Gobet & Simon, 1996b). As expected, there was a strong correlation between skill and recall of game positions ($\underline{r} = 0.68$, $\underline{P} < .001$, 1-tailed). There was also a smaller, but reliable, correlation between skill and recall on the random positions ($\underline{r} = .36$, $\underline{P} < .025$, 1-tailed). Thus, consistent with the prediction of the TT, the prediction of the CAH, and previous data (Gobet & Simon, 1996b), better subjects have better recall on the random positions even with presentation time as short as 5 s.

Are there skill effects in recall of truly random stimuli?

We next focused on whether there were skill differences even on the truly random position type (i.e., the 3/3 positions). Figure 3 (bottom) shows recall as a function of group and position. Recall on the truly random stimuli is generally poor; the mean percentages were 14.8 ($\underline{SD} = 5.14$), 16.3 ($\underline{SD} = 4.47$), 13.7 ($\underline{SD} = 4.95$), and 12.0 ($\underline{SD} = 2.68$) from stronger to weaker players. There was a small but reliable correlation between skill and recall on the 3/3 positions ($\underline{r} = 0.34$, $\underline{P} < .025$, 1-tailed)⁵.

Table 2 shows the results of the multiple regression analyses for all five conditions. On the truly random positions, skill is a significant predictor of recall performance. The skill slope, indexed by the unstandardized parameter estimate, is 0.052 ($\underline{SE} = 0.016$), which compares to TT's prediction of 0.061. Thus, an increase of 100 BCF grading points (e.g., the difference between an average club player and a grandmaster) yields an increase of about 5.2% in recall, which corresponds to about 1.25 pieces. TT predicts that there is a linear

relationship between skill and recall; consistent with this, a quadratic term for skill in a second block in the regression did not significantly improve prediction of recall ($\underline{P} = .19$). Table 2 also indicates that age is a predictor of recall on the truly random positions, with recall declining about 2.6% for each decade of life (age was not, however, univariately associated with recall performance on these positions: $\underline{r} = -0.24$, $\underline{P} = .15$). Visual memory was not a predictor. None of the possible 2-way interaction terms approached significance (all \underline{P} 's $> .37$).

We note that the relationship between skill and recall is a correlation, and the effect size is small. It is possible that other factors, unrelated to knowledge of chess structure, underlie this association. We considered the following possible confounds:

1. Better players have better visual memories in general. There was no evidence for this explanation since there was no correlation between skill and performance on the visual memory task (Waters et al., 2002), and performance on the visual memory task did not predict recall performance on these positions (Table 2). In our sample, the visual memory test exhibited a split-half correlation of 0.76 when the Spearman-Brown correction is used, providing further evidence for the reliability of the measure.

2. Better players were better at using the computer interface. There was no evidence for this explanation since the better players reported being (non-significantly) less skilled at using the mouse ($\underline{r} = -0.18$, $\underline{P} = .28$), and were (non-significantly) slower to manipulate the mouse in reconstructing the opening position ($\underline{r} = -0.19$, $\underline{P} = .29$). There was no correlation between chess skill and preference for viewing 3-D board positions vs. 2-D positions ($\underline{r} = .01$, $\underline{P} = .94$).

3. Better players were more motivated to perform well. The data showed that the better players reported trying (non-significantly) less hard ($\underline{r} = -.16$, $\underline{P} = .34$), being marginally less interested ($\underline{r} = -.32$, $\underline{P} = .054$) and being (non-significantly) more irritated ($\underline{r} = -.22$, $\underline{P} =$

.19) by the random positions. Although these data refer to responses on all random conditions rather than just the 3/3 condition, it seems unlikely that different data would be obtained if effort, interest and irritation responses were elicited from just the 3/3 trials. Thus, differences in motivation are unlikely to underlie the skill effect on the 3/3 condition.

4. Better players guess more, and their guesses are higher quality. Better players may be more willing to guess “blindly” when performing the task and gain a few extra pieces through “pure” guesswork. If participants make lots of guesses, then we would expect them to show large numbers of errors of commission (i.e., incorrectly placed pieces). Table 3 shows that the better players (i.e., the grandmasters, masters/experts) did not make many more errors of commission than the weaker players⁶. The correlation between skill and number of errors of commission on the 3/3 condition was $r = 0.03$, $P = .87$ ⁷.

Skill effects on 2/3, random and 3/3 positions

Table 2 indicates that the skill slopes for the 2/3 condition (0.063), the random condition (0.059) and the 3/3 condition (0.052) follow the ordering predicted by TT. Statistical analyses indicated that the position type x skill interactions for the 2/3-3/3 comparison [$F(1,32) = 0.25$, $P = .31$, 1-tailed] and the random-3/3 comparison [$F(1,32) = 0.11$, $P = .37$, 1-tailed] did not approach significance.

Skill effects on 1/3 positions

Table 2 and Figure 3 indicate that the skill gradient was greater on the 1/3 condition than on other randomized positions. Statistical analysis confirmed that the skill slope on the 1/3 condition was greater than the mean slopes on the other three conditions [$F(1,32) = 4.15$, $P = .025$, 1-tailed].

We noted that the skill slopes in the randomized conditions tended to be less steep than that predicted by TT, particularly in the 1/3 condition (humans: slope = 0.110, $SE = 0.033$ vs. TT: slope = 0.216, $SE = 0.058$). As indicated in Figure 3, the grandmasters did not

perform as well on these positions as TT has predicted (grandmasters: $\underline{M} = 28.1$, $\underline{SE} = 4.1$ vs. TT: $\underline{M} = 53.0$). It is possible that the stronger human players adopted strategies on the random positions which hindered recall performance on the 1/3 positions. For example, some players reported that they just concentrated on a small part of the board (e.g., the first row, or a quadrant of the board) on the non-game positions, and just tried to remember the pieces in these areas. We tested the effect of this strategy on TT's performance by running simulations on which TT fixated only two rows on the board. As expected, this strategy had the effect of decreasing recall performance on the 1/3, 2/3, random and 3/3 positions, and the resulting skill slopes more closely matched the human skill slopes. Since we did not formally assess the strategies that participants adopted, we were unable to carry out more detailed simulations on this issue.

Overall fit between model and data

Figure 4 shows the fit between the predicted scores and the human observed scores, for the 20 data points (4 skill levels x 5 position types). The model captures the human data well ($r^2 = 0.889$, $\underline{P} < .001$, $\text{RMSD} = 6.71$), with respect to both the overall trend and the absolute values. The main deviations occur on the 1/3 positions, where - as discussed above - the model overestimates the human data (on average, by 12.1%), and on the 3/3 positions, where the model underestimates the human data (on average, by 3.7%). Note also that the variability of the human data, and in particular of the model, is low. The fit remains good when the data from the game positions, which were used to match the model with the human data, are excluded from analyses ($r^2 = 0.885$, $\underline{P} < .001$, $\text{RMSD} = 7.21$).

Discussion

The main finding from the experiment was that there was a small, but reliable, skill effect on the 3/3 positions. There was no evidence that this association was driven by any

confounding variable. Thus, the prediction of TT was upheld, and the prediction of the CAH refuted.

TT's prediction that the skill slope on the 1/3 condition should be greater than the slope on the other random positions was also upheld. We also note that the skill slopes on the 2/3, random, and 3/3 conditions did not reliably differ, as was observed in the simulations. Although the experiment may have had insufficient power to detect differences in skill slopes on these conditions, the parameter estimates presented in Table 2 tend to argue against a real interaction in the population. In general, the overall fit between TT and the human data was good. As noted earlier, we used the same version of CHREST as that used by Gobet and Simon (2000) to simulate the role of presentation time on memory recall, with no free parameter adjusted to fit the human data. Hence, the correspondence between model and data supports the chunking mechanisms embodied in CHREST.

The CAH's predictions concerning the relative sizes of skill effects (Game > Random > 3/3, and Game > 1/3 > 2/3 > 3/3) were all upheld. Skill effects were greater in stimuli with more constraint. The CAH would have received further support if the skill slopes on the random stimuli were significantly greater than those on the 3/3 stimuli, and if the skill slopes on the 2/3 stimuli were significantly greater than those on the 3/3 stimuli, since the CAH predicts that "the more constraint available, the greater the expertise advantage can be" (Vicente & Wang, 1998, p. 36). However, as noted above, the experiment may have had insufficient power to detect these differences.

A secondary finding of the study was that age was associated with recall performance on the truly random positions (when controlling for the effects of skill), with younger participants recalling more pieces than their older counterparts. The significance of this finding for the theoretical mechanisms described in TT or the CAH is unclear, but it may suggest that recall performance on these taxing stimuli is facilitated by strategic flexibility or

some other domain-general ability that may favor the young. Whatever the explanation, given that domain-specific knowledge and domain-general abilities may jointly underlie recall performance on the randomized stimuli, the finding underscores the importance of examining the effects of subject variables such as age in this paradigm.

The experiment also brought to light the role of strategic processes on recall performance. As noted earlier, some players reported using a “limited fixation” strategy when responding to the randomized positions, presumably as a cognitive coping strategy. It seems possible that chess players can use multiple processes to encode the randomized stimuli, with the task conditions influencing which process is recruited at any given time. Thus, a master may process a truly random position differently when embedded in a sequence of game positions compared to when presented with other truly random positions.

The study had a number of limitations that should be addressed in future research. It would have benefited from having a larger sample size in order to derive point estimates for the skill slopes with smaller standard errors. As noted above, strategic factors may have influenced scanning and recall of the randomized positions; these strategies could be more fully assessed by eye-movement recordings as well as by post-experiment interviews or questionnaires so that their influence can be further evaluated. Finally, these strategic factors may have had a different impact if the presentation of the randomized positions was blocked by position type; this could be evaluated in future studies.

GENERAL DISCUSSION

The goal of the paper was to test a number of predictions of two theories of expert memory - the CAH and TT - on the recall of randomized chess positions. In particular, our simulation study showed that TT predicts an expertise effect on the truly random positions, whereas Vicente and Wang note that the CAH predicts no expertise effect on these stimuli.

Our human study showed that stronger human players did recall truly random positions reliably better than weaker players, thereby supporting the prediction of TT. Both the CAH and TT make a number of correct predictions about the ordering of expertise effects over the different conditions, although TT over-predicted the expertise effect on the 1/3 positions. In sum, there was support for both theories of expert memory, although TT's prediction prevailed in the truly random condition.

Following Vicente and Wang, we defined the types of random positions by the algorithm which created the material. As can be seen in Figure 2 and the Appendix, this algorithm produced stimuli which violate the most basic board level constraints. Put another way, there is nothing about the statistical properties of the truly random stimuli that should give masters an advantage over weaker players. Thus, Vicente and Wang explicitly stated that the CAH predicts no skill effect on truly random positions constructed like those in our study.

Now that a positive association between skill and recall on the truly random stimuli has been demonstrated, it may be tempting for proponents of the CAH to re-consider how the CAH might be able to accommodate this new finding. For example, as noted earlier, the lowest level for the abstraction hierarchy for chess includes the physical appearance of the board and pieces (Vicente & Wang, 1998, p. 56, Table A2). Since we used conventional representations for boards and pieces, it could be argued that there are appearance-related "constraints" in the truly random stimuli that can be exploited by experts to aid recall (Vicente, 2000, p. 607). However, we would assume that these appearance-related constraints are of equal value to the weaker players, who - like the masters - have presumably only experienced chess environments containing conventional boards and pieces. In lay terms, both masters and weaker players play chess using chess pieces and chess boards that "look the same". Given that the chess environments of stronger and weaker players do not differ at

the level of physical appearance, the better players should not gain any differential benefit from appearance-related constraint.

TT predicts a skill effect in both the random and the truly random stimuli because low-level patterns arise “by chance” in these stimuli, and these patterns facilitate recall in the larger nets. Thus, it could be argued that constraints can also appear by chance and that the truly random stimuli can contain constraints which are exploitable by the experts. But there is no reason to suppose that the algorithm which generates the truly random stimuli will produce stimuli that contain constraints of more benefit to the masters (than the weaker players). In general, the probability of producing a stimulus with even the most board-basic chess constraint (2 kings of opposite colors) is small: we have computed that only one out of thirty-four truly random position will have this property. But it is not clear that this board-level constraint would differentially benefit the masters because this constraint is always present in the chess environments of both stronger and weaker players. In general, in order for the CAH to predict an expertise effect on randomized stimuli in a straightforward manner, there need to be constraints in the test stimuli that reflect the statistical properties of the master-level chess environments. This is the case for the random positions, since these positions contain the distribution of pieces from master-level games. It is also true for the 2/3 positions, since some of the pieces and their location derive from master-level games. But there is no reason to suppose that there are constraints in the truly random stimuli that reflect the statistical properties of the chess environments of master-level players, and thus the CAH - as indicated by Vicente and Wang (p. 45) - does not predict an expertise effect in this condition.

In sum, we have new evidence that in chess - the domain Vicente and Wang (1998) use most often to support the CAH (6 out of the 10 experiments they review) - the CAH does not do better in explaining skill effects in recall than one process theory, TT. We acknowledge that the study only examined expert memory in a relatively narrow range of

conditions, and only in one domain. We also accept that TT did not predict expertise effects perfectly, and that many of the CAH's ordinal predictions were supported. Nonetheless, our findings seem of interest given Vicente and Wang's (1998) claim that process theories "cannot provide an adequate theoretical explanation for expertise effects in memory recall for the considerable number of domains in which memory recall is a contrived task" (pp. 34-35).

The theoretical explanation for recall on this contrived task invokes several mechanisms. First, by learning chunks and templates, CHREST captures regularities in the environment. Second, templates are necessary to explain the huge skill superiority on the game positions, and, to some extent, on the 1/3 positions, over the other types of positions. Third, attentional mechanisms are important, as was shown by the simulations of the 1/3 positions. Finally, a limited visual STM is necessary to account for most of the simulations; with an unlimited capacity, little skill effect would be found as the information about individual pieces could simply be stored in STM. Thus, through a variety of cognitive mechanisms, CHREST provides an adequate theoretical explanation for expert recall on a contrived task.

Nonetheless, we recognize that cognitive mechanisms distinct from those proposed in this paper may mediate the skill effects observed in this study. For example, masters may be faster at encoding the features necessary to identify the symbols for pieces. Thus, a faster "piece-detector" or a piece-detector that operates in parallel over a larger visual field could conceivably contribute to skill effects on the randomized stimuli. (Simulation studies in TT in which encoding time is manipulated across skill levels could help determine the impact of this low-level mechanism). Note also that chess skill itself only explained a modest amount of variance in the recall of truly random positions (around 10%); presumably there are (as yet undiscovered) domain-general factors that are associated with recall on these positions. For example, participants may use an assortment of strategies during the performance of the task

(e.g., Siegler, 1999), particularly under the more taxing random conditions, and individual differences in strategy use may plausibly explain some of the variance in recall. Further work is required to more fully understand the multiplicity of mechanisms – both domain-specific and domain-general - that underlie recall performance on the randomized chess positions.

It is important to recognize that the ultimate goal of TT, within the chess domain, is to model human chess play. Our underlying assumption is that pattern recognition is the key process underlying chess skill. The critical idea is that TT learns patterns (chunks) not with the explicit goal of excelling in memory experiments, but to use them as conditions of productions that may be useful later in playing chess (e.g., Gobet & Jansen, 1994). As with the chunking theory, expert performance in recall tasks is a “side effect” of acquiring goal-oriented knowledge for performing a task at a high-level. That is, the chunks are an invaluable, if inadvertent, aid to performance in the recall task (Simon & Gobet, 2000).

When comparing the CAH and TT, it is important to distinguish between the process/product and the formal/informal dimensions, which are orthogonal. The CAH is clearly a product theory, and TT a process theory. TT, implemented as a computer program, is a formal theory. Its implementation, CHREST, makes quantitative predictions, sometimes about small differences, which is certainly preferable to predictions of ordering - the most informal models can do (cf. Grant, 1962; Meehl, 1967). The CAH is mostly formulated informally, except for the DURESS program which specifies an abstraction hierarchy for thermal hydraulic process (Vicente & Wang, p. 53-55). In this case, the hierarchy allows more powerful predictions than the informal hierarchies proposed in other domains, such as chess. In particular, it is possible to manipulate several constraints in parallel and to make quantitative predictions. Thus, the desirability for formal theories applies both for process and product approaches.

In the same way as the CAH can be used both formally and informally, TT can be applied to tasks even when no simulations are available. When used as a verbal theory, the EPAM family of models, of which TT is a member, can be shown to account for a large amount of data in other domains of expertise (cf. Simon & Gobet, 2000), including domains in which memory recall is a contrived task. That said, given that TT is a process theory, it is ultimately optimal to construct computational models for all of the other domains in order to derive quantitative predictions in these other domains (Vicente, 2000). Indeed, the scope of CHREST is being extended to language acquisition, memory for computer programs, and physics expertise (Gobet et al., 2001).

In sum, despite the wealth of recent theoretical articles on the nature of expert memory (e.g., Ericsson, Patel, & Kintsch, 2000; Simon & Gobet, 2000; Vicente & Wang, 1998; Vicente, 2000), to the best of our knowledge this is the first study to undertake a detailed empirical analysis of the relative merits of the CAH and the TT. Our data suggest that a process theory makes a number of correct predictions for expert recall in chess. Thus, although much further testing is required, process theories may be able to provide an adequate theoretical explanation for expert memory in contrived tasks.

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Footnotes

¹For now, the reader should note that we collected recall data on four groups of human participants with mean British Chess Federation (BCF) ratings of 235.6 (grandmasters, the strongest group), 201.2 (masters/experts), 150.9 (Class A/B players), and 112.3 (Class C/D players, the weakest group). As described in more detail later, the mean recall on the game positions for the four groups was 83.7%, 61.3%, 55.1%, and 40.1% respectively. As indicated in the text, these percentages were used to select four nets (displaying similar recall); the recall performance of these nets on the random positions was further examined.

² The World Chess Federation (FIDE, *Fédération Internationale des Echecs*) publishes rating lists of its members every six months and awards the following titles, in descending order of merit: grandmaster, international master and FIDE master. The BCF rating is an interval scale ranking competitive chess players in the UK. Skill levels have standard names, which are used in this paper (in parentheses, the approximate corresponding range in BCF points): grandmaster (normally above 240), international master (225 - 240), master (200-225), expert (175-200), class A players (150-175), class B players (125-150), and so on. Chess ratings tend to be stable between years in adult players; in our sample, the correlation between the BCF Aug 1997 and Aug 1998 grades was $r = .99$.

³Details of administration and analysis of this test are presented in Waters et al. (2002).

⁴This experiment is part of a study where both randomization and the location of pieces on squares (pieces on center of squares vs. pieces on the intersection of squares) were manipulated. Presentation of standard and intersection positions was blocked and the order of presentation counterbalanced over participants. Here we report data from the standard positions; the results from intersection positions will be reported in a separate paper.

⁵The correlation between skill and recall for the 2/3 condition was $r = .33$, $P < .05$, 1-tailed. For the 1/3 condition, the correlations were $r = .46$, $P < .005$, 1-tailed.

⁶The large means and standard deviations for the errors of commission in the 3/3 condition in Table 3 were caused by a few participants who appeared to guess prolifically; 7 participants placed at least five more incorrect pieces than correct pieces (on average) on this condition. These participants ($N = 7$, $M = 13.6\%$, $SD = 6.4$) did not perform better on the 3/3 position types than the non-guessers ($N = 29$, $M = 14.6\%$, $SD = 4.1\%$, despite having (non-significantly) higher ratings (guessers, $M = 186.9$, $SD = 36.4$ vs. non-guessers, $M = 174.1$, $SD = 47.3$).

⁷Pure guessing is unlikely to greatly improve recall on the truly random positions. We simulated the effects of guessing by determining the amount of pieces that would be “recalled” correctly through two guessing methods; guessing from the legal distribution of chess pieces at the beginning of a game (i.e., one white king, one white queen, eight white pawns, etc.), and guessing from a rectangular distribution. The estimates were based on 1000 positions with an average of 24.6 pieces. Both methods showed that putting down 32 pieces on each truly random position would yield one piece correct, and putting down 10 pieces gives an expected yield of 1/3 of a piece correct. Of course, the kind of guessing that participants will use in the recall task is likely to be more sophisticated than pure guesswork. As an example of this, one grandmaster verbalized after one random trial that he knew there was not a knight in the position. He was not sure what there was, but he was sure there was not a knight. If he were then to guess, he could use his knowledge of the absence of a knight to constrain his response, and so this would not represent pure guesswork. Thus, better players’ “guesses” may be of higher quality than weaker players, which may be partially explained by the larger chunks they can access (see Gobet & Simon, 2000). However, an investigation of this issue was beyond the scope of the present study.

Table 1.

Mean values (SD in parentheses) of the characteristics of the four groups

	Grandmasters	Masters/ Experts	Class A/B	Class C/D
	<u>N</u> = 7	<u>N</u> = 12	<u>N</u> = 10	<u>N</u> = 7
BCF rating, August 1998	235.6 (5.6)	201.2 (14.8)	150.9 (11.9)	112.3 (9.1)
FIDE rating, July 1998	2493 (44)	2245 ^a (98)	2045 ^b	^c
Age	34.7 (3.8)	28.7 (8.2)	25.5 (9.3)	25.6 (5.8)
Sex	7 M	10 M, 2 F	9 M, 1 F	7 M
No. "slowplay" games/past yr	67.6 (49.8)	46.4 (31.4)	20.7 (13.5)	28.3 (14.3)
No. "rapidplay" games/past yr	20.9 (27.6)	25.2 (27.3)	19.3 (17.1)	7.6 (5.3)
Days since last slowplay game	22.4 (19.9)	38.6 (24.0)	63.6 (39.2)	44.1 (31.3)
Studying time (hrs/week)	16.4 (13.7)	6.4 (9.1)	3.7 (3.2)	1.6 (1.7)

Note. "Slowplay" games normally involve each player having > 75 mins thinking time for a certain number of moves. "Rapidplay" games are those where the participant has around 30-40 minutes thinking time for the whole game. ^aThis constitutes an average of 11 players; one expert did not have a FIDE rating in the July 1998 list. ^bThis refers to the rating of one Class A player who had a FIDE rating in the July 1998 list. ^cNo Class C or D player had a FIDE rating.

Table 2.

Results of multiple regression analyses in which skill, age and visual memory are entered together in one block

Position Type	R ²	BCF rating	Age	Visual Memory
Game	50%	0.35*** (0.063)	- 0.50 (0.37)	- 0.45 (0.40)
1/3 Randomized	31%	0.11*** (0.033)	- 0.22 (0.19)	0.33 (0.21)
2/3 Randomized	26%	0.063** (0.022)	- 0.29* (0.13)	0.09 (0.14)
Random	17%	0.059** (0.023)	- 0.16 (0.14)	- 0.09 (0.15)
3/3 Randomized	28%	0.052*** (0.016)	- 0.26** (0.09)	- 0.06 (0.10)

Note. For each position type, a regression was performed by entering BCF rating, age and visual memory into the same block. For each regression, the R² and the unstandardized b coefficients for the three predictors are shown (1 SE in brackets). On the truly random positions, an increase in 100 BCF points yields about a 5.2 % increase in recall.

* $\underline{P} < .05$, ** $\underline{P} < .01$, *** $\underline{P} < .005$. For BCF ratings, \underline{P} values derive from one-tailed tests.

Other \underline{P} values reflect two-tailed tests.

Table 3

Mean number of errors of commission (i.e., number of pieces placed incorrectly) by position.

Position Type	Grandmasters	Masters/ Experts	Class A/B	Class C/D
	<u>N</u> = 7	<u>N</u> = 12	<u>N</u> = 10	<u>N</u> = 7
Game	2.43 (2.37)	4.48 (3.22)	4.34 (2.78)	3.94 (3.08)
1/3 Randomized	4.00 (4.44)	2.55 (4.24)	4.44 (4.83)	2.40 (2.61)
2/3 Randomized	4.40 (5.85)	3.15 (4.82)	5.70 (6.78)	2.69 (2.67)
Random	4.31 (5.07)	3.15 (4.09)	5.98 (6.71)	3.17 (1.93)
3/3 Randomized	6.14 (7.75)	3.28 (4.51)	6.04 (7.47)	2.91 (2.00)

Note: SD's are shown in parenthesis.

Figure Caption

Figure 1. Methods used to generate the various types of random positions.

Figure 2. Examples of stimuli used in the experiment.

Figure 3. Percentage of pieces correctly recalled as a function of type of position and number of chunks in the discrimination network for the model (upper panel) and of skill level for the human data (lower panel).

Figure 4. Predicted (x axis) and observed (y axis) percentage recall on the different conditions. The vertical error bars indicate the standard errors of the mean for the human data. The standard errors of the mean for the model (not shown in the graph) are all less than 1%.

1. Random positions: the pieces of a game position are randomly assigned to an empty square in the new position

2. Truly random positions: Not only the location, but also the type of piece are randomized. The following algorithm is used:

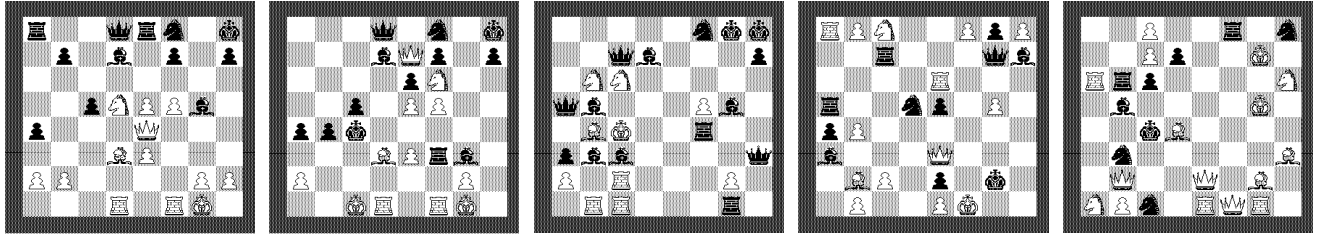
For the desired number of pieces, do:

1. pick up an empty square randomly with equal probability in the new position;
2. pick up a type of piece, with equal probability, from the set of 12 black or white types of pieces;
3. assign that piece to that square

3. Truly-random-1/3: with equal probability, randomly keep 2/3 of the pieces on their location. Using the algorithm in #2, truly-randomize the other 1/3 pieces

4. Truly-random-2/3: with equal probability, randomly keep 1/3 of the pieces on their location. Using the algorithm in #2, truly-randomize the other 2/3 pieces

Figure 1



Game

1/3 Truly Random

2/3 Truly Random

Random

3/3 Truly Random

Figure 2

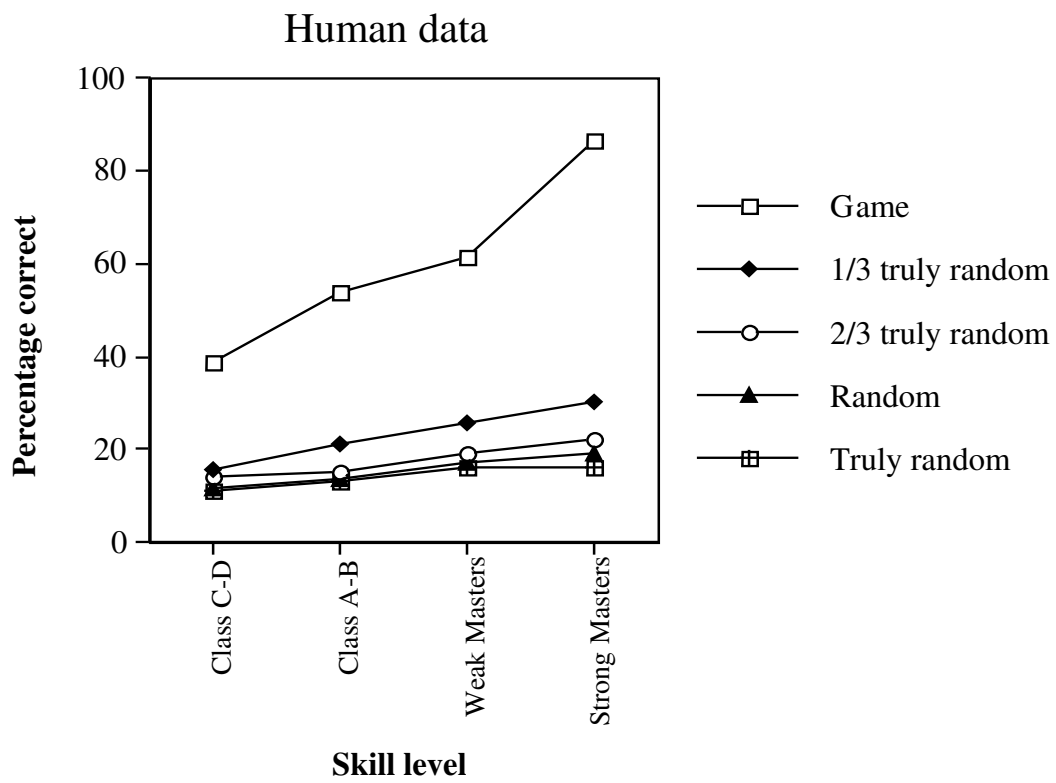
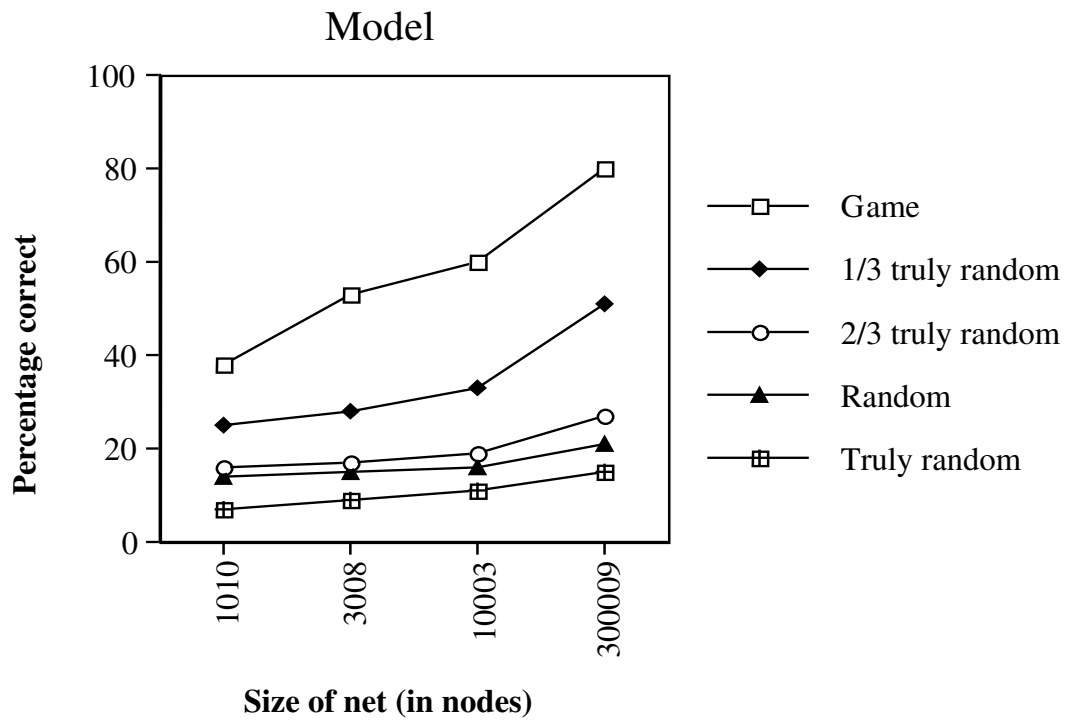


Figure 3

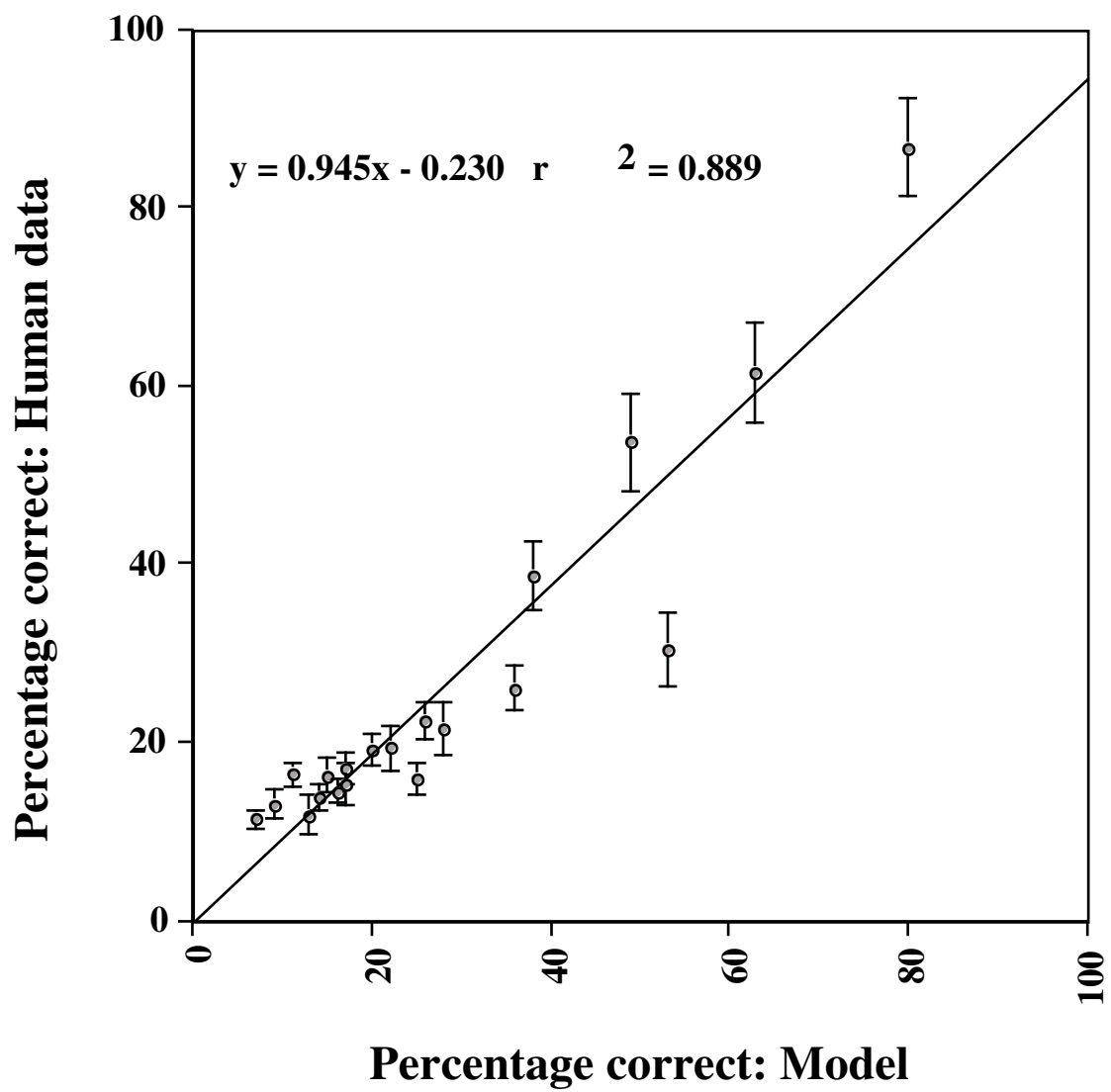


Figure 4

Appendix: Positions used in the 3/3 condition

The positions are given in Forsyth notation. In this notation, the board is scanned rank by rank from a8 to h8, then from a7 to h7, and so on to h1. Letters indicate pieces (uppercase for white, lowercase for black). Digits refer to the number of empty squares between pieces or the board's boundaries. Ranks are separated by slashes.

Position 1: Pb2bBqP/2B1k3/1pRK2pB/1Q2K2b/R1p3q1/k5r1/P2Q2b1/5p1p/

Position 2: 4bqP1/8/r2kkKrK/1r4b1/1qNrn2N/2b1Bb2/Q5Bn/1r2n1kP/

Position 3: 1n1Q1RQ1/4P1q1/1n1Kk2r/qN4P1/Q7/B5Q1/4bNpK/N1R2p1Q/

Position 4: b1q5/1k1q4/2BQ1k1P/5K2/3p3b/2nN1qKK/k3B2N/1B2pPrB/

Position 5: qQ1QR1nn/K1pr4/1n1R1k2/6q1/7n/1BN5/K3PpBq/b3qBr1/

The composition of the truly random stimuli is given in the following table. Note that the frequency of each piece type is reasonably close to the expected frequency, which is 2.08 (25 pieces / 12 types). The table also indicates the expected frequency for each piece in master game positions, computed from a sample of 10,000 positions after 20 moves. It can be seen that all truly random positions seriously break the statistical constraints of master games.

Table A1.

Frequency distribution of white and black pieces for the five truly random stimuli used, and for a sample of 10,000 master games after 20 moves.

	White							Black							
	K	Q	R	B	N	P	sum	k	q	r	b	n	p	sum	Sum
Truly random stimuli															
position 1	2	2	2	3	0	3	12	2	2	1	4	0	5	14	26
position 2	2	1	0	2	2	2	9	3	2	5	4	3	0	17	26
position 3	2	5	2	1	3	2	15	1	2	1	1	2	2	9	24
position 4	3	1	0	4	2	2	12	3	3	1	2	1	2	12	24
position 5	2	2	2	3	1	1	11	1	4	2	1	4	2	14	25
Mean	2.2	2.2	1.2	2.6	1.6	2.0	11.8	2.0	2.6	2.0	2.4	2.0	2.2	13.2	25.0
Master-level games															
Mean	1.0	0.8	1.9	1.3	1.1	6.1	12.3	1.0	0.8	1.9	1.3	1.2	6.2	12.4	24.7